

Anthropological Collaborations in Colombia

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Colombian ethnographers have for decades engaged in a committed anthropology marked by political and social collaboration, blurring the lines that separate what their North American colleagues call applied anthropology, advocacy, and pure research (Jimeno 1999; Rappaport 1990). In the past few decades, the communities that Colombian anthropologists traditionally studied have begun to do their own research, forcing a shift in the terms of dialogue between external scholars and communities, as well as in the expectations that communities have of researchers. In this chapter I examine the way an engaged Colombian ethnography emerged over time and tease out of its history a sense of what collaboration has meant to Colombian researchers.

I focus on social scientists whose work with popular movements has revolved around the construction of a research agenda, as opposed to those whose relationships with social movements have been confined to advocacy. I look especially at the development of participatory action research in the late 1960s and the 1970s by Orlando Fals Borda and La Rosca de Investigación y Acción Social (Circle of Social Research and Action) in collaboration with Colombian indigenous and peasant organizations, and at the work of the anthropologist Luis Guillermo Vasco in the 1980s with the history committee of the indigenous community of Guambía. Vasco's approach, unlike La Rosca's, privileged the field as a site of co-theorizing between academics and grassroots researchers. In considering how these scholars saw the research process transformed by collaboration, I home in on the locus of theorizing, the nature of agenda building, and the creation of horizontal relationships between researchers and community.

The kind of collaborative research conducted by Colombian academics contrasts with many examples of collaborative ethnography in North America. Contemporary North American anthropologists have written extensively about the significance of collaborative writing (Hinson 2000; Lassiter 2005; Lawless 1993; Ridington and Hastings 1997), but such writing does not purport to transgress the boundaries between applied and “pure” research. Rather, its fundamental goal is the production of a new kind of ethnography geared largely to a scholarly readership—that is, a new kind of pure research.

In contrast, Colombian anthropologists’ attempts at collaborative research break down these boundaries in crucial ways. Their commitment to collaboration is tempered by an equally significant engagement with activism. Thus their research is simultaneously pure and applied, though perhaps such distinctions do not cut to the core of what this research is about. Unlike the collaborations of most applied researchers, these scholars’ collaborations with popular organizations are essentially political, geared toward promoting social justice from the grassroots, which makes their research more activist than applied. At the same time, their commitment to community-based research agendas results in work that is not pure in an academic sense, because it is not driven by academic theory (Strand et al. 2003: chap. 4)—although, as I demonstrate, they also produce innovative scholarly publications. Finally, what most significantly distinguishes these Colombian activist-scholars from their North American colleagues is their focus on field practice as a space in which social analysis and political action can be generated, as opposed to an emphasis on writing as a vehicle for scholarly collaboration.¹

This Colombian collaborative heritage bears implications for research conducted by foreigners in Latin America, as well as for the work of nationals. Indeed, it provides us with an alternative intellectual lineage that we can engage in building a new anthropological agenda for the twenty-first century. In the second half of the chapter, I bring this lineage to bear on my own collaborative relationship over the past decade with the Regional Indigenous Council of Cauca (CRIC), a major Colombian indigenous organization.

Initially, CRIC members sought me out after having read my published and unpublished work on the colonial history of the Nasa ethnic group (Rappaport 1998). I was first asked to coordinate history workshops in local communities with the objective of facilitating the collective study of colonial reservation titles whose contents had passed into the oral domain and had become a kind of mythic charter for communities

and for CRIC. Later, I was invited to participate as a team member in a study of the history of CRIC's Bilingual Intercultural Education Program (PEBI). In the process, I discovered that collaborative research does not revolve exclusively around the production of a written text by a research team. Instead, it requires extended exchange with a broader range of activists, transcending the project itself. At the same time, it constitutes a key space for generating a new kind of anthropological theorizing that spans the divides between pure and applied research, between the academy and those undertaking research within popular organizations.

My approach to ethnography is heavily indebted to these Colombian researchers. Although it has resulted in scholarly writing, I have attempted to follow the Colombian example by shifting my emphasis from writing to analytical dialogue in the field. This has led to products that are of utility to the organizations with which I have worked and has contributed even more to the development of a distinct grassroots research methodology. In this sense, my notion of what ethnography is and the ways in which it can be harnessed in an activist context has been permanently altered. The link between theory and ethnographic interpretation has moved, for me, away from being forged through solitary academic pursuit and toward fostering an appreciation of the way collaborative research can promote co-theorization with the communities we study. Reflexivity has come to be redefined, in my experience, as a process of contemplating the epistemology of our theoretical dialogue. This has reshaped my methods, my interpretive strategies, and the contents of my research, redefining ethnography for me in significant ways.

The Militant Research of La Rosca

In 1972, a book titled *Causa popular, ciencia popular* (Popular cause, popular science) appeared in Colombia, published by La Rosca de Investigación y Acción Social, a network of Colombian social scientists and journalists advocating militant research alongside popular movements (Bonilla et al. 1972). La Rosca members proposed abandoning the university—or at least rejecting the traditional research methodologies of the academy—to employ their scholarly skills in the service of popular sectors by inserting themselves as researcher-activists into local and regional struggles. They argued in favor of establishing research priorities in conjunction with local militants, studying the history of the militants' organizations, and

then returning the results of their research to them (Bonilla et al. 1972: 44–46).

Participatory action research, as this approach came to be called, presupposed “that the researcher himself is an object of investigation: his ideology, knowledge, and practice are judged in light of popular experience. The exploitation that occurs when people are studied as ‘research objects’ (a veritable sacking of their cultural values and of the treasure-house of their experience) is abandoned, leading to a respect for them, their contributions, their critiques, their intelligence” (Bonilla et al. 1972: 46, my translation). Final authority would rest with the popular sectors and not the researchers (1972: 47). La Rosca proposed, in essence, that popular sectors “expropriate” scientific knowledge, techniques, and methodologies (1972: 48).²

La Rosca’s task did not conclude once its research was completed. Its members advocated a methodology that they called “critical recuperation,” which turned research results toward activist ends:

Critical recuperation is achieved when, on the basis of historical information and an adequate understanding of current conditions, militant researchers arrive in communities to critically study and learn about the traditional cultural base, paying special attention to those elements or institutions that have been useful in the past to confront the enemies of the exploited classes. Once those elements are determined, they are reactivated with the aim of using them in a similar manner in current class struggles. (Bonilla et al. 1972: 51–52)

Local peasant practices of labor exchange and institutions such as the *resguardo* (or reservation, an institution that ensures collective land rights for native peoples) and the *cabildo* (the *resguardo* council, which operates as a semi-autonomous entity) were the sorts of practices La Rosca hoped to study and subsequently reintroduce into communities (1972: 52).

The best-known of La Rosca’s efforts is the publication of a treatise by Manuel Quintín Lame (1971 [1939]), a Nasa leader who forged a land-claims movement in the Colombian southwest during the first half of the twentieth century (Castillo-Cárdenas 1987; Rappaport 1998). The posthumous appearance of Lame’s writings reactivated his memory among the Nasa of Cauca and the Pijao of the neighboring department of Tolima, helping to spur the Regional Indigenous Council of Cauca (CRIC), founded in 1971, to include in its organizational objectives Lame’s demands for reclaiming land within

the *resguardo* structure and for strengthening *cabildos* (Avirama and Márquez 1995). La Rosca members produced historical pamphlets reviving the memory of eighteenth-century hereditary chiefs who created *resguardos* in the region (Bonilla 1977), and they introduced a series of picture maps through which local history could be collectively recaptured (Bonilla 1982).³ The products of La Rosca's research, although not academic, would not have appeared if not for the intervention of these activist-scholars. The continued significance of these publications in indigenous organizations—where they are still studied and used by people at the grassroots—points to the possibilities of a collaborative model that transcends academic writing.

La Rosca also produced some academic documents, such as Orlando Fals Borda's *Historia doble de la costa* (Double history of the coast), a four-volume experiment in narrating the history of the agrarian struggles of people living along Colombia's Atlantic coast (Fals Borda 1980–86). *Historia doble* is organized in two channels, one narrative and the other theoretical and methodological. Fals intended the narrative channel to be consulted by leaders of agrarian organizations and the theoretical channel to be read by politically committed intellectuals and academics. A series of comics, pamphlets, and audiovisual materials was to be produced for the rank and file of the social movements in the region (Bergquist 1990: 160).

The ethnographer Luis Guillermo Vasco made an insightful critique of La Rosca's methods with the hindsight that came from his collaborative experience in the indigenous community of Guambía in the 1980s. He argued that the politically committed scholars of the 1970s made the positivist error of separating field practice from the theoretically informed interpretation of data—the error, precisely, of the organization of Fals Borda's history of the Atlantic coast (Vasco Uribe 2002: 454–57). As a result, he said, La Rosca's attempt to “return” its research to the communities in which it had worked was not entirely successful; La Rosca members did not recognize the position of power they occupied as scholars or that they, like other researchers, had constituted the groups they studied as objects of analysis (2002: 457–58). Ultimately, many aspects of La Rosca's project were academic in their intent and unassimilable (at the time) by the social movements with which they were concerned (Bergquist 1990; cf. Fox 2005).⁴ In short, La Rosca's political commitment did not preclude its adherence to traditional ethnographic procedures or its insistence on employing theory originating in the academy.

Theorizing and Co-theorizing

The African American sociologist W. E. B. DuBois argued in *The Souls of Black Folk* (1989 [1903]: 2–3) that racial minorities in the United States keenly perceived that they at once stood within the North American social system and were marginalized by it. This produced what he called a “double consciousness,” a dual identity that carried the potential of granting a privileged perspective on social life, which he termed “second-sight.” Following DuBois’s lead, African American scholars have taken up the challenge of producing theory, something that as early as 1970 Delmos Jones (1970: 251) argued would be crucial in creating a “native anthropology.” The scholars I have in mind, particularly the anthropologist John Langston Gwaltney (1981, 1993) and the feminist Patricia Hill Collins (1991), proposed that such a body of theory could arise only out of collective reflections with common folk who, drawing on their life experiences, could theorize in collaboration with academics.

La Rosca’s project involved workshops of the sort used by Gwaltney and Collins, but La Rosca’s community meetings were directed primarily toward establishing research priorities and collecting information, not toward generating theory out of local cultural forms. Indeed, workshops are a prime vehicle for social research in Colombia (Riaño-Alcalá 2006), constituting a practice that permits information gathering, collaborative strategizing, and political action simultaneously. This format contrasts with the tendency among North American ethnographers to privilege one-on-one interviews, which control the interview situation to a greater degree than is possible in the communal setting of a workshop.

Workshops, however, are a necessary but not sufficient tool for promoting theorizing among ethnic minorities and other popular groups. The generation of theory from below became possible in Colombia only in the 1990s, after a layer of indigenous activists was trained in structural linguistics at Colombian universities and began to experiment with the use of linguistic forms as conceptual guides in cultural research.⁵ It was necessary for them to learn what theory was and why it was needed before they could subvert the theory learned in the academy.

One of the explicit areas of activity of the members of CRIC’s Bilingual Intercultural Education Program (PEBI) is theoretical production. The theorists in CRIC’s ranks are indigenous linguists and students of cosmovision—a kind of a politicized take on cosmology—because it is from indigenous languages and shamanic knowledge that these activists derive their theoretical building blocks. By theory, they mean

the conceptual vehicles that guide the analysis and interpretation of cultural forms and the construction of strategies for revitalizing cultural practices and reintroducing them into social life. Although such conceptual frameworks may originate in indigenous cultures, they are also, as I illustrate later, appropriated from the theoretical traditions of the dominant culture and recast according to native priorities.

Theory may be employed to guide the production of narrative, as I describe shortly for the Guambiano History Committee (which is affiliated not with CRIC but with the Indigenous Authorities of Colombia [AICO], another indigenous organization) and for the history of PEBI. But theorizing also takes place when activists use language and cosmovision to coin new definitions of citizenship that are incorporated into their political strategies, a process reflected in the translation of the 1991 Colombian constitution into indigenous languages (Ramos and Cabildo Indígena de Mosoco 1993; Rappaport 2005). The generation of theory is therefore both an intellectual exercise and an activist objective. Theorizing is a means of imbuing political organizing with cultural significance, a process necessarily punctuated by moments of research and reflection. The ultimate objective, however, is political, not intellectual. In other words, this is a kind of “situated theorizing” (Haraway 1991) in which not only does intellectual practice emerge out of the theorists’ ethnic positioning but, more importantly, political activity is guided by their research.

In instances in which external scholars collaborate with members of indigenous organizations on research projects, we might also speak of “co-theorizing,” or the merging of differently situated theories. As I describe in the following section, co-theorizing involves the forging of connections between indigenous-created concepts and the theories and methodologies that politically committed academics draw on from their own traditions. Indigenous theoretical frameworks are privileged in this process, but they are expanded through the contributions of external theory and method, so that the conceptual frameworks of both the indigenous researcher and the professional anthropologist are transformed.

Theorizing in Guambía

The Guambiana linguist Bárbara Muelas Hurtado has argued that the rolling and unrolling of geographic and social space governs the relationships that Guambiano families establish with their surroundings: “When one or more people who live in a house are invited to leave for

other sites, they are invited with the expression *pichip mentøkun*, which literally means 'let us unroll.' The opposite situation, when they are invited to return home, is *kitröp mentøkun*, which means 'let us roll up' or 'let us collect ourselves'" (Muelas Hurtado 1995: 32).⁶ Similarly, this action of rolling and unrolling defines Guambianos' relationship with time:

As the years unfold, in lived time, in the voyage of life and through the world, the ancestors have marked a path, they have opened a trail on which those who come behind, their descendants of today, must advance to make history. The past goes in front, and the future comes behind. It is as though our ancestors had returned to look for their descendants or as though those who have already left (died) had returned to "judge" what their descendants have accomplished in their absence. It is as though a turn were made in the vast circular space, a new meeting with the ancestor, [of] past and future, an illusion, a hope, in Guambiano thought.

The space before us and lived time (conserved in tradition) orient human life. They go ahead [of us] in life, and [continue] after life. The space that is left behind and time not yet lived are a space and a time that must still unroll. (Muelas Hurtado 1995: 35–36).

Muelas's linguistic anthropology would form the basis for the creation of a Guambiano conceptual space in which time is conceived as a spiral and history is narrated in a nonlinear, spiral fashion (Vasco Uribe, Dagua Hurtado, and Aranda 1993; see also Rappaport 2005: chap. 5).

It was in a collaborative project organized by the Guambiano History Committee, in which Bogotá anthropologists Luis Guillermo Vasco and Marta Urdaneta participated, that Guambiano theorizing came to fruition. The history project was conceived after the Guambianos reclaimed a large expanse of land in 1980 that had earlier been usurped from them, which they ultimately incorporated into their *resguardo* territory. The research was aimed at establishing the primordial claim of the Guambianos to that territory. Although title to the land is vested in the *cabildo* and is in no danger of being revoked, local non-indigenous historians had for decades argued, without any evidence, that the Guambianos originated in Peru and were relative newcomers to Guambía. The *cabildo* hoped to deploy historical research to debunk these theories and establish the Guambianos' centuries-old attachment to their territory. Rigor was thus at a premium. Vasco and Urdaneta were inserted into research teams composed of elders knowledgeable

in oral history and young Guambianos eager to learn to do research. The external scholars concentrated, respectively, on oral history and archaeology, topics of study dictated by the indigenous authorities.⁷

The methods espoused by these interethnic teams were collaborative in both the collection of materials and their analysis. Urdaneta, for example, worked with her Guambiano colleagues to develop classifications in Guambiano of historical time and topographic space, which were then employed as guides for establishing an archaeological chronology and, ultimately, for generating an interpretation of Guambiano prehistory that explained the nature of Guambiano territoriality (Tróchez Tunubalá, Camayo, and Urdaneta Franco 1992; Urdaneta Franco 1988). Vasco and his Guambiano co-researchers developed theorizations of the shape of time modeled on Guambiano material culture, which provided a theoretical foundation for a narrative of land loss, forms of capitulation and resistance, and territorial claims in Guambía (Vasco Uribe 2002: 466–73).

Unlike standard anthropological research or even La Rosca's approach, the work in Guambía subordinated the professional ethnographer's methods and epistemology to those of the indigenous collectivity (Vasco Uribe 2002: 449). This subordination is one of the hallmarks of what I call "co-theorizing." The oral history project encompassed large groups of people who reflected in public assemblies on the historical narratives of elders. Vasco wrote that he was initially confused by the procedures used at these meetings, where breakout groups seemed to mull interminably over discussion points without reaching firm conclusions, a process that was repeated in plenary sessions. Ultimately, he concluded that what appeared to be aimless discussion was, in fact, purposeful:

The work in breakout groups organized by indigenous people in their meetings was, in reality, a research meeting, in which knowledge of a problem was intensified through discussion, in which they confronted the knowledge of every participant with that of the rest in order to finally arrive at group knowledge... My idea that there were no conclusions at the meetings was wrong; there were conclusions, but they did not take the same form as those with which I was familiar, nor were they written. Later, it became clear to me that after the breakout groups and the multiple discussions that ensued in them, in the mind of every participant lay certain conclusions: a broader knowledge of the problem than there had been before the meeting, now that it was no longer personal knowledge but knowledge held by the entire group. (Vasco Uribe 2002: 461)

Vasco had to learn to “roll and unroll” at these assemblies, because this was what the Guambiano researchers saw themselves as doing. This meant also walking the territory with the elders (Vasco Uribe 2002: 303), using toponymy as a guide to learning the past (2002: 295–96), and conceiving of history and territory as unrolling from a central topographic point (2002: 293). The result was a historical narrative based in the topography of Guambía and organized as a spiral that continually sighted and resighted on primordial events. The spiral format, a topographic manifestation of Bárbara Muelas’s notion of “rolling and unrolling,” provided the theoretical framework through which this history was organized.

It would be too simplistic to suppose that Urdaneta’s and Vasco’s only contributions to the project were technical. True, the Guambianos mined the two scholars for their scientific knowledge and methods, as La Rosca had proposed two decades earlier.⁸ But the anthropologists also functioned as interlocutors, co-investigators, and co-theorizers. Vasco emphasized that the texts they wrote were dialogic: “Our objective was to build dual texts, at once oral and written, at once indigenous and Spanish, where the various authorities each had their place and fulfilled their roles within the needs of the moment” (Vasco Uribe 2002: 318). Although for the most part the distinctive voices of the outsiders are muted in the seamless pamphlets published by the *cabildo*, in at least one piece—which appeared in an academic anthology—the language of the anthropologist was interjected into the historical narrative (Vasco Uribe, Dagua Hurtado, and Aranda 1993), laying bare the moments at which Vasco’s contributions became critical. This text has abrupt transitions between oral narrative and ethnographic explanation, the latter drawing out the implications of what is expressed in the narration. This ethnographic voice appears to be Vasco’s and provides important clues to the way co-theorization took place: the ethnographer’s eye helped to alert Guambiano theorists to the implications of the conceptual frameworks they had derived from linguistics, thus deepening their interpretations.

It would be a grave error to focus exclusively on the exceptional texts that the collaborative teams produced. Most publications of the Guambiano History Committee are less elegantly written and theorized than the article just cited (see also Dagua Hurtado, Aranda, and Vasco 1998). The vast majority are less layered, less explicitly theoretical, and composed in more accessible language. They were self-published by the *cabildo* for use in schools, where the major concepts are recast in the Guambiano language, returning them to the oral domain. Moreover,

the most important contributions of the project were never written at all. As Vasco (2002: 462; see also Riaño-Alcalá 2006) contended, the team's major objectives were achieved through the development of a collective ethnographic research methodology, not through the creation of ethnographic texts. In the long run, the purpose of this methodology was to infuse Guambiano social life, especially in the political and educational spheres, with a self-conscious notion of culture in the hope of revitalizing native lifeways.

In this sense, Vasco's (and the Guambianos') understanding of ethnography as practice contrasts with the North American emphasis on ethnography as text, the latter approach suited to the academic objectives of our craft but not to the collaborative enterprise. But I can take this comparison even further. Vasco did not reduce practice to the collection of research data (as La Rosca did) but focused instead on generating theory and applying it to the analysis of materials collected during the research. He also focused on how this knowledge was brought to bear in resolving the social problems the community faced. That is, ethnography became an exercise in political action.

Research in the Intercultural Milieu of the Indigenous Organization

The experiences of Vasco and Urdaneta in Guambía suggest that what is at stake in collaboration is the bridging of epistemological and methodological differences in the service of a political agenda. The collaborative research process had to admit the possibility of theorizing from a Guambiano cultural standpoint if it was to be successful. Indeed, this is what makes the project so inspiring. But the bridging of cultural differences is not exclusive to the Guambiano-academic divide. Equally significant is the bridging of differences between the culture of the academy and that of indigenous organizations that serve as umbrella groups for native peoples of various ethnicities who do not necessarily share epistemologies. As anthropologists, we have been trained to study the kind of epistemological difference that Vasco and Urdaneta encountered in Guambía, but we are less attuned to the far subtler cultural order of the multiethnic indigenous organization, whose contours appear self-evident because its organizing strategies hold so much in common with those of other social movements.

In order to get at this organizational culture—which is crucial if we are to comprehend collaborative research within the indigenous movement—we must examine the social dynamics of research within such

organizations. This task implies a comprehension of the interculturality of indigenous politics, including the articulation of non-native supporters of indigenous organizations—in CRIC, they are called *colaboradores*—and their role in the research process.⁹ *Colaboradores'* positioning as “researchers within” suggests that it is not only indigenous culture that is at stake in the collaborative research enterprise but also the intercultural milieu of the organization, in which political methodologies are adapted to research ends and research is transformed into political strategy. The presence of *colaboradores* in the research process problematizes what co-theorization might mean, because although they are not indigenous, they are central actors in the emergence of indigenous theory and form an integral part of the indigenous movement, forcing us to reconsider where activist theorizing originates.¹⁰ I use my experience in CRIC as a guide to exploring these issues.

When CRIC militants speak of research, they do not refer to the long-term, argument-driven collection and analysis of information in which academics engage, the kind of work that ultimately crystallizes on the printed page. Instead, they refer to communal spaces of reflection, intuitive processes, organization-building activities, and the production of tools for political action. Although the results of academic research may resonate in many ways with this plan of action, and scholarly researchers may be enthusiastically engaged as interlocutors, the methods and objectives of the two groups are quite distinct.

Vasco's reflection on his difficulty in interpreting the results of Guambiano assemblies points to the essentially communal nature of indigenous research in Cauca. The workshops in which research priorities are determined are generally quite large, sometimes involving up to a hundred people, and include participants with varied experience in the organization who belong to diverse generations of activists and exhibit an uneven command of indigenous languages. After plenary sessions in which the leadership presents background information and proposals, assemblies generally divide into breakout groups to consider discussion questions provided by the coordinators.

When too many breakout group members are young or inexperienced, or when the leader adheres too closely to the discussion questions, the result is a disjointed series of one-sentence responses elicited from the discussants one by one and then reported back to the plenary by schooled participants or by *colaboradores* in what could be interpreted as a translation into movement discourse. When there is some consistency in the membership of the breakout groups, however—for instance, when members share common organizational experience

and are not afraid to speak out, something I have seen happen when participants are mostly bilingual teachers or catechists—lively debate and stimulating conclusions are presented to the plenary, leading to a spirited reinterpretation of the smaller groups' findings. At its best, the workshop strategy results in a powerful blueprint for research and action that is harnessed in the political activities fomented by the workshop, as well as in a host of materials ranging from curricula to pamphlets informed by collective discussion.

Collaborative research, then, not only involves working in a team with activists but also permitting the communal environment of the workshop to guide the research. Activist-researchers frequently possess (or are open to learning) the skills needed to frame research questions and to gather and interpret information. But workshop participants are not researchers in any standard sense. Notwithstanding this fact, in organizations such as CRIC it is in large assemblies that research guidelines are frequently set and the results of research evaluated. Therefore, as academics involved in collaborative relationships with indigenous organizations, we cannot lose sight of the interpretive space that workshops afford, nor can we ignore the suggestions made by their participants. Collaboration thus begins to take on broader proportions.

Guidelines for a History of PEBI

In order to work through this issue, I reflect on a project in which I participated, charged with preparing a history of CRIC's Bilingual Intercultural Education Program (PEBI). Although I was influenced by the work of La Rosca and the Guambiano History Committee—I was attuned to the necessity of collectively establishing a research agenda and to the significance of co-theorizing—CRIC's methods and concerns played an important role in shifting my notion of the purpose and nature of ethnographic research.

When I embarked on the project, I naively assumed that our research would be conducted within the confines of the research team, supplemented by workshops at which communal interviews would be conducted and the results of our research evaluated. I discovered, however, that a series of workshops held in 2000 and 2001—before and immediately after I joined the project—had already compiled the research guidelines. In total, approximately one hundred people attended at least one of these meetings. The majority were indigenous bilingual teachers or PEBI activists, but leaders in the communities

where the workshops were held also attended, since most such meetings are hosted by localities.

The goal of the earlier workshops had been to define the project and devise a research plan, including the generation of research questions. Initially, the group decided it would conduct a systematic survey of the activities and accomplishments of the program's various components, which included curriculum development and primary research in cosmovision and linguistics. Such a detailed critical inspection is called a *sistematización*, a genre that is widespread in popular education and alternative development planning in Latin America. A *sistematización* is a combination of descriptions of the organization's activities and the results of collective evaluation of issues facing the group. As I understood it on the basis of previous examples of this genre produced by CRIC (2000), the *sistematización* was a compendium of raw data organized into categories, followed by a brief evaluation of the project; its written results were conveyed in a largely undigested and somewhat schematic format.

When I began work with the project, I questioned the utility of the *sistematización* format because of its unmanageable final product. I was also attentive to the critiques I had heard from many Nasa-speaking activists who preferred embarking on a history project, largely with the aim of learning a new set of research methodologies. In contrast, most of the *colaboradores*, who had administrative experience in a variety of organizations, opted for *sistematización*, which was more familiar to them.

As I soon discovered in the first workshop I attended, the Nasa speakers intended to wrest control of the project from the *colaboradores*, underlining the fault lines within the organization that would impinge on our research. Ultimately, the history format won out, thanks to the critical mass of Nasa speakers at the workshop. But even though the group opted for a historical approach, the research questions that were to guide our work were essentially guidelines for a *sistematización*.¹¹ There were fifty-one questions, the vast majority of them presentist in orientation, in large part evaluating key PEBI policies that had made education a fulcrum for community organizing and had fostered the development of a self-conscious notion of indigenous culture. Few of the questions were retrospective.¹²

Reconciling History with *Sistematización*

The research team, made up of a Nasa linguist (Abelardo Ramos), a *colaboradora* (Graciela Bolaños), and me, was obliged to build a bridge

between the *sistematización*-oriented questionnaire given to us by the workshop participants and the history we hoped to narrate. Three concepts emerged from the guiding questions to become pivots of the historical narrative we eventually published (Bolaños et al. 2004). First, we focused on the development of the school as a community organizing tool rather an institution devoted exclusively to education. This is what CRIC calls the concept of community control. Second, we looked at the way cross-cultural dialogue and the appropriation of ideas across cultures—which we called interculturalism—provided a philosophy for building a pluralist movement.¹³ Finally, we studied the way cosmovision—a concept that articulates the need for a balanced universe with a return to indigenous ritual forms, which developed out of seminars held among shamans in the 1990s—furnished a new pivot for identity construction at a moment when the movement replaced its class-based objectives with a politics of peoplehood. In other words, our conceptual framework originated in none of the individual cultures of indigenous groups in CRIC but in the culture of PEBI as an organization.

Unlike the guiding questions, we did not situate PEBI policies in the present but used the guidelines as a lens for interpreting the past. That is, while recognizing that interculturalism and cosmovision were approaches that had gained currency in the past decade (community control was a guiding concept from the beginning of PEBI), we believed they could still serve as powerful hinges for making sense of the way early activists harnessed external ideas and struggled to revitalize cultural forms. By organizing our history around these notions, we were able to situate historical narration in the present, making it more meaningful for contemporary indigenous readers and providing a three-channeled reading of the history of PEBI.

The participants in the workshops that produced our research strategy attended just a few of the interviews we conducted with key regional and local historical actors. They were not active in the writing phase; Ramos, Bolaños, and I did the writing as a team, negotiating the narrative line by line.¹⁴ Although the history we wrote was of an education program, the two CRIC team members, following the directives of the workshops, insisted that it be more political than pedagogical in scope. This taught me an important lesson about CRIC's research priorities and the nature of CRIC's educational programs. In the early stages, I had envisioned a spiral history, given that Nasa activists, like the Guambiano historians and Vasco, had embraced this motif as central to their cosmovision (Anonymous 2000). Nevertheless, Bolaños and

Ramos felt that the use of a spiral organizing model would reduce our narrative to a Nasa-centric history, ignoring the interculturality of the program and the participation of activists from other ethnic groups in the project. Their strong intercultural stance is what led us to adopt a conceptual focus based on organizational culture as opposed to one of the various indigenous cultures represented in CRIC.

Socialización

Although the authors of the research guidelines did not help to write the history, their insights were again at a premium when the book was completed and we began what in CRIC is called *socialización*, or the collective grassroots evaluation of a project. Thus, in a sense, the book was but a pretext for two important arenas of organizing that took place immediately before and after its publication. At a 2004 meeting to plan the *socialización* workshops directed at bilingual teachers and community leaders, which began in 2005, PEBI activists probed the relationship between the conceptual structure of the book and CRIC's political objectives. They were largely concerned with the tension between PEBI as an education program and PEBI as a political organization.¹⁵

On one hand, our interlocutors proposed to delve into what they saw as a dialectical relationship between pedagogy and politics. They sought to comprehend how their creation of intercultural pedagogical methodologies constituted a political act. They wanted to reflect on what tools PEBI history offered to stimulate the evaluation of the teaching methodologies and didactic materials they had generated over the course of more than twenty years. On the other hand, they were keen to interpret ways in which the three conceptual axes of the study could be reconciled with CRIC's general political program, which promotes the defense of the communal land base, the deepening of political autonomy, and the promotion of indigenous culture. In particular, they questioned how they were to understand autonomy in light of the strategy of appropriation of external ideas that characterizes interculturalism and how this might impinge on indigenous culture. In summary, what PEBI activists hoped to do with our book was to turn it into an organizing tool whereby local teachers and community leaders reflected on the program's objectives at yet more workshops. The book was by no means a conclusive study but a small step in a communal organizing process.

The Role of the Anthropologist in Collaborative Research

Like Urdaneta's and Vasco's roles in the Guambía history project, my role in the PEBI project was grounded in the methodological contributions I could make to the team: sharing my skills as an interviewer and assisting in writing a coherent and persuasive narrative. But these were not the only contributions I made, nor did the PEBI collective conceive of my collaboration in such narrow terms. To conclude this chapter, I offer my reflections on what else I contributed to the enterprise and how it transformed my notion of ethnographic practice.

Earlier I cited a La Rosca text asserting that collaborative research transforms "the researcher himself [into] ... an object of investigation: his ideology, knowledge, and practice are judged in light of popular experience" (Bonilla et al. 1972: 46). In other words, collaboration implies a kind of "being-looked-at-ness," which the film critic Rey Chow (1995: 180) argued is at the heart of the autoethnographic endeavor. For Chow, autoethnographers are simultaneously the subject and the object of their research; the fact that they have been examined ethnographically by others informs the way they look at themselves. I have reflected in other publications on how "being-looked-at-ness" is central to the ethnography produced by Nasa intellectuals (Rappaport 2005: chap. 3). What I examine here is how it is also a fundamental element of collaboration, forcing the external ethnographer to look at herself, just as indigenous participants do—a brand of reflexivity that transcends the inward-looking and individualistic thrust of North American cultural critique, which Charles Hale (2006a and this volume) has contrasted with activist anthropology.

I went into the PEBI history project accepting that although historical narration is situated in the present, we would need to harness current experience to a retrospective interpretation of the past. Ramos and Bolaños did not entirely understand or subscribe to this premise, because it meant evaluating the past as something different from the present—as somehow, from their point of view, politically imperfect. For instance, as we evaluated narratives of PEBI's early attempts to lodge the educational project in indigenous lifeways, we confronted the fact that the incipient understandings of culture as a constellation of customs that characterized PEBI in the early 1980s clashed with current notions of an integrated cosmovision that permeates all aspects of everyday life and political practice. The latter implied that the earlier efforts had not been entirely successful, something Ramos and Bolaños were reluctant to accept.

At the same time, because we were charged with harnessing the research guidelines supplied to us by the workshops, we had to employ current concerns with cosmovision, community-based education, and interculturalism to interpret the past. Now wearing the hat of the historian, I was initially uncomfortable doing so. My self-appointed task was to lay bare the way I thought past and present could be reconciled, forcing Ramos and Bolaños (and me) to evaluate the research strategies I was accustomed to using and to try to place them in dialogue with the ideas articulated by CRIC. In this sense, I was being looked at by them and, subsequently in the *socialización* planning, by the PEBI collective.¹⁶

The “being-looked-at-ness” of my collaboration with CRIC resided not only in the way its members looked at me but also in how I looked at myself as a researcher—the extent to which I needed to transform my future research practice to conform with their methodologies. In a sense, then, “being-looked-at-ness” on the part of the external collaborator is a kind of reflexivity. Unlike the reflexive turn in anthropology, however, which focuses on the ethnographer herself, the kind of reflexivity I came to practice homes in on the process of co-theorizing that I first learned of through the work of Vasco and the Guambiano History Committee. Co-theorizing turned my reflexive sensibilities not toward myself but toward the dynamics of the group of which I was a member. This reflexive posture not only involved confronting the difficulties I had in subordinating my more solitary interpretive strategies to the collective approaches of CRIC; it also meant learning to adopt my colleagues’ theorizations in my own research, so that interculturalism moved in both directions.

Such a transformation did not take place exclusively in the scenario of writing PEBI’s history. It also happened when I sat down at my computer and composed my own essays, which would be read by both CRIC activists and my academic peers. In this sense, collaboration resulted in a dialogue in which CRIC was not the object of my research or my assistance but another interlocutor, thus expanding the intellectual networks through which my work traveled and from which I derived intellectual sustenance. Such an approach diverges from the premises of applied anthropology, which is a one-way process emanating from the anthropologist and directed toward a client population. To achieve my collaborative goal, I could not entirely subordinate my methods to those of CRIC but had to confront the two in my research and writing practice, using what I learned from Ramos and Bolaños to both question and expand on the academic premises upon which I worked.

Only then could we engage in a true collaboration involving mutual respect and dialogue.

The three concepts that framed our project all came out of the intercultural milieu of the indigenous organization. Accepting this framework was for me a significant step, because it forced me to discard the essentialist baggage I brought to the project—not just my notion of what constituted “indigenous” or “Nasa” but also my notion of what an indigenous organization was all about and who belonged to it. I came to see that it would be impossible to study indigenous organizing without taking account of the place of the indigenous movement in the larger Colombian political universe and of the border-crossings of its diverse members. This led me to recast my understandings of identity formation, informed by the anthropology of the north to focus exclusively on ethnic subjects. I came to understand that the fluidity of ethnicity in Colombia goes beyond such concerns, taking on a distinctly political tinge that is deeply constructivist, not essentialist. Even when CRIC activists speak of something like Nasa culture, they are pointing to political strategies, not existing states of being. I do not think I would have come to understand this point if not for the collaborative experiences I have enjoyed over the past decade.

Collaboration is thus a transformative experience. It not only affects the organizations with which external scholars work, making possible a deeper commitment to politically inspired research, but also leads those scholars to rethink their own craft, to roam beyond the academy in search of theory and method, to embrace a new layer of interlocutors, and to rethink the ways in which they reflect on themselves and others. In short, collaboration cannot be seen as a paternalist application of anthropological skills in a practical context; it must be seen as an innovative vehicle for revitalizing anthropology in the twenty-first century.

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Notes

1. I distinguish in this chapter between a North American insistence on the primacy of ethnography as a genre of writing and a Colombian focus on ethnography as field practice. This distinction does not preclude the significant point made by Gelya Frank (this volume) that both pure and applied forms of anthropological research in the United States are seen largely as exercises in writing. My argument regarding the nature of Colombian participatory action research also coincides with the discussion of activist anthropology advanced by Charles Hale (this volume).

2. The participatory action research of La Rosca is not to be confused with the participatory approach currently in vogue in rural development (Chambers 1997; see also Cooke and Kothari 2001). Although the latter advocates grassroots participation in development projects, it does not involve the sort of collaborations with radical organizations that were central to La Rosca's objectives, nor do its aims revolve around bolstering the capacity for militancy of the grassroots, as La Rosca advocated. In addition, building on the influence of Marxism in the Colombian social science of the 1960s and the wide participation of students and scholars in leftist parties and movements, La Rosca sought to imbue its research with a class (and later an ethnic) consciousness that is generally missing from the panorama of rural development practitioners.

3. Víctor Daniel Bonilla, a member of La Rosca, was active in CRIC at its founding. I was initiated into the Colombian indigenous movement (and introduced to its intense factionalism) in the late 1970s by Víctor Daniel, who

also encouraged me to begin to analyze the archival materials I had collected by juxtaposing them with the histories prepared by La Rosca. Gonzalo Castillo, also of La Rosca, was made aware of Lame's book by the indigenous community of Ortega, Tolima, where the manuscript was carefully stored and consulted by native authorities (Castillo-Cárdenas 1987). Lame's treatise has since been reissued (Lame 2004 [1939]); the new publication includes an indigenous take on Lame's biography, rendered in comic-book form (Nene and Chocué 2004: 103–10).

4. In a sense, Bergquist's critique is unfair. Although La Rosca's pamphlets, books, and picture maps were inaccessible to many indigenous activists in the 1970s, they have become accessible today, when there is a significant layer of schooled native militants who are not only readers but also identify themselves as researchers. In March 2006, at a meeting of the CRIC leadership to discuss strategies for writing an organizational history, participants continually brought up these materials.

5. An MA program in ethnolinguistics was developed at the Bogotá-based Universidad de los Andes in the 1980s, with the participation of international scholars as instructors, through a combination of French and Colombian funding. Indigenous activists were provided with scholarships and studied alongside non-native, tuition-paying students. The program spawned other MA programs in provincial universities, including the Universidad del Cauca, and attracted indigenous linguists trained in more traditional linguistics programs.

6. The $\emptyset$ is used in the Guambiano alphabet for vowels originating at the center of the tongue, like the French *u*.

7. The history committee also collected extensive archival materials from a number of Colombian and Ecuadorian repositories. However, its members did not have the skills to read and assimilate these documents, and the hundreds of pages of copies they amassed went unconsulted.

8. The Guambiano History Committee explicitly recognized the relationship between research skills and power, as it stated in one of its narratives: "When the Spaniards came, they arrived doing research. They grabbed those who lived in Cajibío, whom they called Novirao, and they taught them Spanish. They asked them about everything. They called those who knew the most and they asked them, so as to learn all there was to know about the Guambianos. When they had found out everything, they began to dominate them, to exact tribute, to organize things and people in their own way. They sent the hereditary chiefs to collect everything they wanted, to take it to Popayán [the colonial capital]" (Vasco Uribe, Dagua Hurtado, and Aranda 1993: 9).

9. *Colaboradores* are for the most part former leftists who sought out the indigenous movement as a prime site for constructing a new proposal for

nationhood in Colombia. Like the members of La Rosca, they hoped to promote the class struggle in the countryside, although today's movement has come to espouse a discourse of ethnic peoplehood that *colaboradores* have also embraced. However, unlike La Rosca members, *colaboradores'* primary identity is that of activists and not researchers.

10. The quandary over where to place *colaboradores*—whether they belong “inside” or “outside” the movement—hints at the essentialist lens through which we have become accustomed to viewing indigenous organizations.

11. CRIC is a multilingual environment in which members of various indigenous ethnic groups and *colaboradores* communicate largely in Spanish. Only half of the Nasa are fluent in the Nasa language. Many of the most radical proposals for cultural revitalization come from the Nasa speakers within the PEBI collective, who are frequently at odds with Nasa activists who are monolingual in Spanish and with the *colaboradores* (see Rappaport 2005).

12. The fifty-one questions are analyzed in Rappaport 2005: chap. 5.

13. Interculturalism is a pedagogical movement that advocates the appropriation of external theories and methods into an indigenous cultural matrix in order to stimulate research and, ultimately, foster political pluralism (López 1996). An antidote to multiculturalism, it is meant to empower subaltern groups, as opposed to simply tolerating their presence.

14. One of the obvious skills that external researchers bring to the table is the ability to write up results in a coherent and persuasive fashion, something that the activists—whose writing skills were confined to curricula and political documents—could not do. Vasco and Urdaneta also functioned as scribes for their research teams.

15. *Socialización* activities have been ongoing, mainly in the teacher training programs coordinated by PEBI. However, in August 2005 I attended a workshop organized to begin to conceptualize a broader history of CRIC, at which we engaged in a *socialización* of the PEBI book. Participants included not only PEBI activists and bilingual teachers but also the founders of CRIC—men and women in their sixties and seventies—who had been charged with writing a history of the organization. Given the objectives of the workshop, the contents of the book were not discussed in any detail; instead, participants used the PEBI history to project their goal of writing a history of CRIC. They reflected on how the moments in PEBI's history recorded in the book shed light on ways to think about the history of other localities. They were concerned, in particular, with methodological issues and with the possibilities of training elders to participate in historical research. Moreover, they emphasized the significance of interpreting history through vehicles other than published books, including visual images and video. Finally, they spent a great deal of time elaborating on the purpose of a CRIC history: to ignite the organizing

flame under an increasingly apathetic youth—a very different objective from that of the PEBI history, which was to stimulate further research among bilingual teachers.

16. Ramos and I are currently working on a dialogic article about our experiences on the team, to be published by *Historia Crítica* in Bogotá. In it, Ramos narrates his own confrontations with the team and lays out what he learned in the process.